Author's Response

Mundari and argumentation in word-class analysis

by NICHOLAS EVANS and TOSHIKI OSADA

1. Introduction

Our three commentators raise such a host of deep and interesting issues that we cannot hope to answer them all within the time and space at our disposal. To begin with, we would like to thank them for pushing us to articulate the reasons for our arguments more clearly, and for getting us to spell out a number of assumptions and intermediate argumentative steps that we did not make sufficiently clear or explicit in our original article. We will deal with the three commentators one by one, limiting ourselves to a few key points.

2. Peterson

We begin with Peterson's commentary, which is the most straightforward, and focus on two key points in his argument. He broadens the empirical base by bringing in another Munda language, Kharia, but we believe that the essential points of our analysis apply there as well. (We also have some disagreements with him about the Mundari data he cites but since they are peripheral to the main argument we do not go into them here.)

The crucial flaw in his critique of our analysis is his failure to take on board the criterion of bidirectionality. In Section 3.3 of our original article we make it clear that merely showing that languages are generous in what they allow to be predicates does not demonstrate a conflation of word class distinctions: we also need to go back and look at whether all words under discussion can be employed in argument slots. (Recall that, as we point out in the target article, it was a shift to this bidirectional requirement by Jacobsen, Schachter, and Anderson that underpinned the counter-analysis to Swadesh's monocategorialist

Linguistic Typology 9 (2005), 442–457 1430–0532/2005/009-0442

view of Nootka.) Yet all of Peterson's examples, in both Kharia and Mundari, are limited to the question of what words are used as predicates. Without addressing the converse question, of what words can be used as arguments, his refutation of our analysis simply falls short.

His point about the difference between phrasal and lexical categories is an important one, and many of his Kharia examples (such as (8b)) suggest that a wider range of morphological elements following the predicate are in fact predicate enclitics rather than suffixes, positioned with respect to the last element of what may be a phrasal predicate (such as *ho rocho$^{?}$b* 'this side'). Treating them as clitics to predicate position readily allows us to explain the acceptability – and prevalence – of a wide range of phrasal elements in predicate position.

In Section 1 of our article, we mention that "though the elements following the verb in examples like these have generally been regarded as suffixes, they are in fact less tightly bound to the root, phonologically, than other suffixes are". It would not be an outrageous analysis to go further and treat them as enclitics, along the lines of Peterson's analysis – i.e., to restrict the use of "suffix" to what we called "close" suffixes, and call the outer suffixes "predicate enclitics". We could then analyse these clitics as attached to phrase-level constituents, as per Peterson's analysis, but leave "inner suffixes" (such as the passive *-o*) to operate at the lexical level. And it may be an appropriate way to deal with the very common construction type in which inchoative or stative meanings are composed with phrases used in predicate position – and perhaps also with the "quotative" meaning exemplified in his example (12).

However, it would be misleading to think this analysis would then account for all examples where basically entity-denoting lexical items are used in predicate slots. As we show in the section "Corollary: Compositional consistency", the productive semantic patterns by which nouns attract stative or inchoative readings when used as intransitive predicates, and factitive meanings when used in transitive predicates (e.g., 'pancake' > 'make a pancake'), are not the only types of semantic increment we find when we look over the whole lexicon. We also find 'use as an X', for example (e.g., 'scissors' > 'cut with scissors'). All Peterson's examples illustrate the primary pattern whereby predicate use produces the inchoative, stative, or quotative readings, and we cannot comment on whether Kharia is semantically more constrained than Mundari in the range of possible semantic increments. But certainly for Mundari the sort of treatment he proposes will not work once the full set of semantic conversion effects needs to be accounted for.

3. Hengeveld and Rijkhoff

Hengeveld & Rijkhoff (henceforth H&R) effectively want to propose a further way that a language could lack a noun-verb distinction, not included in our list

of four – their "flexible" type – and want to locate Mundari within it, along with other languages such as Samoan. They admit, though, that these types are idealized and that Mundari has some features of their "Type 2". This continues the thrust of previous publications by these authors and their associates.[1] We do not find their arguments for this position convincing, for reasons we now spell out.[2]

Firstly, from the outset they impose the rider "as far as its basic, non-derived vocabulary is concerned". An analysis of the word classes in any language must be comprehensive, and include ALL lexemes, not just the non-derived ones. We have already discussed the dangers of not requiring exhaustiveness in Section 3.4 of our paper and need not repeat these arguments here, except to underline that exhaustiveness is just as important in terms of covering the basic vs. derived dimension as it is in terms of covering the full set of lexical items in each semantic domain being surveyed.[3]

The heart of their objection lies in their imputation of vagueness to the semantics of Mundari lexemes (Section 3.2.1). To make their analysis work, they would need to do two things.

Firstly, they would need to succeed in stating what the putative "vague" meaning is; without an explicit representation of this, putative vagueness analyses are mere promissory notes (cf. our critique of precategorial analyses in

1. As H&R point out, we do not refer in our article to a further recent publication of theirs on the topic, which also draws on Mundari data (Hengeveld, Rijkhoff, & Siewierska 2004). This omission reflects the chronology of our paper, submitted to *LT* in 2004.
2. We will not address the issues raised in Section 5 of H&R's article, dealing with purported typological correlations between Mundari and other "flexible languages". This is because we do not regard this as a coherent notion, for reasons we spell out below, nor are we convinced by their arguments that Mundari should be analysed as such a language.
3. H&R again discard the exhaustiveness criterion in including Samoan as not having a distinct class of nouns (Section 1, just before example (7)), in this respect following the analysis of Mosel & Hovdhaugen's authoritative grammar. As their quote from Mosel & Hovdhaugen shows, there are in fact words in Samoan that do not appear to be used in argument function (e.g., *alu* 'go') and others that do not appear to be used in predicate function (e.g., *mea* 'thing'). Unlike H&R, we do not find either of Mosel & Hovdhaugen's reasons for downplaying the significance of these exceptions convincing. The first of M&H's reasons is that "we cannot find any functional explanation" (for these differences) – this is not a valid counter-argument, we maintain, because (i) lack of motivation is the best type of evidence for combinatorical restrictions being conventionalized and not semantically based, and (ii) in any case, observation/description is a separate logical step to explanation, and it is a sufficient goal of a good reference grammar to furnish explicit descriptions, without having to explain every phenomenon (cf. Dryer forthcoming). The second of M&H's reasons has to do with the provisional nature of the data they were working from, in other words, that their rules were restricted to the corpus they were working with. But this is a general constraint on all descriptions – all formulations of grammatical rules are limited to the corpus at hand, and in that sense provisional – and should not therefore be used to downplay the significance of exceptions, especially when they involve quite common and basic lexemes.

Section 2.2). We find their arguments here quite unconvincing. Firstly, they do not propose a semantic analysis for any Mundari lexeme. Secondly, even for the Samoan example they discuss (*lā* 'sun, be sunny, sunny' – p. 415) they do not attempt to state what the proposed vague meaning is, and within the schematized representation of meaning components they give (in terms of unspecified meaning components A, B, C, D, and E), there is no single element found in all three uses.[4] This certainly does not meet the normal requirements for demonstrating the existence of a shared meaning, even on the formulation they give themselves in connection with a true case of vague meaning such as (p. 414): "we can find an abstract definition that covers both interpretations: male cousin and female cousin" (e.g., one assumes, in a formulation like 'parent's sibling's child' – NE & TO).

Secondly, they would need to show that the meaning of words used in particular syntactic contexts accrues the same semantic increment, across all lexemes of the class, in the same compositional way.[5] This is not a requirement they accept – see their discussion in Section 3.2.2 – but we believe theirs to be a mistaken position, for reasons we now elaborate.

On p. 416 of their comment they remark:

> in our opinion E&O's discussion of the problem of semantic arbitrariness fails to appreciate the difference between conceptualization and lexicalization and, as a result they seem to treat metaphor as something different from normal language.

This criticism rests on a failure – common in the cognitive linguistics literature – to give due weight to the difference between lexicalized (conventionalized) and creative metaphor. Of course metaphor and other tropes are not something different from normal language, and of course figurative speech is "an integral part of human language and categorization" (p. 416). However, once a metaphor or other trope has become lexicalized it has created a particular semantic increment that is part of the conventionalized lexicon of the language

4. A further problem is that the standard tests for vagueness vs polysemy that they cite are all set up to deal with lexical items that occur in the same syntactic position (e.g., *Duffy discovered a mole, and so did Clark*, where the tested item is an object NP in both cases). Adapting these to deal with the case at hand, where one meaning occurs in a predication slot and another in another argument slot, is a non-trivial task. About the closest we can come to testing this is by using an infinitive slot, embedding the tested lexeme under a verb capable of taking both an infinitive and a base noun, like *laga* 'be tired of' or *caba* 'finish'. If we do this with a word like *jom* 'food; to eat', in a sentence like *jom=ko lagatana* or *jom=ko cabajana* we find that it is in fact not vague: these mean, only, 'they are tired of/have got tired of the food' and 'they have finished the food' respectively. To express the action meaning we need to use a reduplicated form of the word at hand, which normally has an iterative meaning, e.g., *jojom*. Thus: *jojom=ko lagajana* 'they have got tired of eating'. It is difficult to account for this effect if one adopts a vagueness analysis. (We thank Maki Purti for this additional data.)
5. For Croft this position is still too weak – we take this point up below.

it occurs in, that must be listed – and defined – as a conventional and arbitrary fact about THAT lexeme, in that language.

A relation of metaphorical or metonymic extension may hold in three ways:

(i) between a conventionalized meaning, part of the stable mental lexicon of all or most members of the relevant speech community (and dictionaries that record this knowledge), and a creative extension in context, which is part of an "utterance meaning" but is not conventionalized.

(ii) between different senses of a word, that are combinatorically equivalent (e.g., their example of *mole*); here we have a case of a lexicalized polysemic link. An example would be the conventionalized use of a noun to mean 'many' as well as 'mountain'. For a polysemic link (ii) to be found, of course, it must previously have been used by some creative speaker, as in (i), then passing from pragmatics to semantics via a process of structuration, variously known in this case as grammaticalization, lexicalization, or depragmaticization – see, e.g.. Hopper & Traugott 1993, Evans 2003.)

(iii) between signs that differ in their combinatorics as well as their meaning, with a lexicalized relation of heterosemy (Lichtenberk 1991). This, as we argue in Section 3.2 of our article, is the best analysis of the relation between Mundari *buru* '*n.*: mountain' and *buru* '*v.t.*: heap up, pile up' or *jom* 'v.t.: eat' and *jom* '*n.*: 'food', which have distinct combinatorics in addition to the rather complex semantic relation between the respective senses. In fact, the semantic relationship between the linked senses here involves more than one step in each case, so that a better example would be *laTab* 'n.: scissors' and *laTab* 'v.t.: cut with scissors', where there is a straightforward metonymy of the type 'entity' > 'perform characteristic action using entity as instrument'.[6]

Even though there are a number of widespread crosslinguistic tendencies in the realm of figurative speech, the tendency to confuse trends with universals in cognitive semantics needs to be reined in and tempered with proper consideration of the range of alternative figurative pathways that languages manifest (see, for example, the arguments in Evans & Wilkins 2000 against treating as universal certain patterns of extension from 'see' into the realm of cognition).

---

6. Once we distinguish these three situations, another of their objections to our analysis evaporates, namely the issues of whether there is "a principled way to distinguish across languages between zero conversion and polysemy" (p. 417). To qualify as zero conversion there must be a shift in combinatorics, whereas polysemy is a relation between signs with (i) the same signifier, (ii) semantically related signifieds, and (iii) the same combinatorics. Of course, the issue of combinatorics is intimately tied up with the whole analysis into word classes, so there might appear to be a danger of circularity here. But since the solution to the word class problem must satisfy multiple constraints – those we elaborate in our article – this is not a fatal problem.

It is an arbitrary and conventional fact about Mundari, which must be learned, that *kapi* participates in the linked meanings 'axe (i.e., an entity)' and 'form into an axe, make an axe' in Mundari while its English counterpart *axe* participates in the linked meanings 'axe (i.e., an entity)' and 'cut, get rid of'. To be sure, in each case an original creative process of entity → process metonymy gave rise to a figurative extension, in an individual utterance. But the subsequent processes of lexicalization then led to the selection of different metonymic links in Mundari and English, each absorbed into the respective langue or lexical system of each language. Though we agree that, in certain cases, "it is impossible to draw a hard and fast line between literal and non-literal language", the cases we discuss are safely on the side of lexicalized use (which is why they appear in dictionaries of these languages), and should not be confused with creative tropes, which are free to be created on-line, free of pre-existing conventions in the language.

For this reason we regard any attempt to dispense with the requirement of regular semantic increment as a misguided confusion of the difference between pragmatics and semantics, and would not accept any analysis of word classes in a given language as justified if it fails to measure up on this requirement. Since, additionally, their proposed counter-analysis does not even state the "vague" meanings explicitly, we do not regard it as a convincing treatment of the Mundari facts – nor would we regard as satisfactory an analysis of any other language along the same lines. It follows that, on our view, their "flexible" language type remains an underdefined construct, unless it simply corresponds to what we call a "rampant zero conversion language" – and, in that case, as we argue in Section 2.4, the proliferation of differing semantic increments accompanying zero conversion are too diverse to make it possible to identify precategorial lexical meanings, so that we are driven back into setting up distinct word classes after all.

We must also address H&R's disagreement with our characterisation of Hoffmann's position on word classes in the *Encyclopaedia Mundarica*. They are quite correct to cite his brief statement on pages 8–9 saying he has "nothing to add" to his earlier *Introduction*. However, what is striking is the exhaustive, semantically explicit, and scrupulously accurate listing of attested uses for each lexeme – which is of course what makes *EM* such a fabulous resource for the study of widespread zero conversion. But in addition to cases where he explicitly states that particular uses are impossible (such as the statement about *aŋ* 'dawn' (*EM* p. 120) quoted by H&R) there are many others where he only lists nominal or only verbal uses, such as *atiri* (p. 257) 'sbst., a cover of an earthen waterpot' (*EM* p. 257) or *araŏ* 'trs. to tie the special knot by means of whih the … carrying net … is attached to the … carrying pole' (*EM* p. 164), from which a nominal can only be formed by infixation: *a-n-araŏ* 'vrb. n., the manner of tying this knot'. Whether he explicitly excludes a particular use, or only does

so implicitly by not listing it, what is striking is the honesty and accuracy of his coverage: he only puts in uses when he has attested them, and his data correlates amazingly well with what we have been able to check with Maki Purti and other Munda speakers.

It is this vast body of robust and detailed data and the labellings he uses with it, rather than the brief statement in his introduction, which we were wanting to contrast with his earlier claim. Moreover, we disagree with H&R that "what he is actually describing is the meaning of a lexical item when used as the head of a referential phrase, without committing himself to classifying the item in question" (as a noun, verb, etc.). In fact he is conscientious about giving a large number of examples in their phrasal or sentential context, and in any case our reading of the passage immediately preceding the section H&R cite (from pages 8–9 of Vol. 1 of *EM*) suggests he is in fact drawing much more on the distributional criteria (of infixation and suffixation) than H&R's quote suggests:

> The bulk of the language consists of monosyllables coalescing into the words that make up a sentence. In this work prefixes and infixes play the main part in the formation of the new words, whereas SUFFIXES AND, TO SOME EXTENT, INFIXES ARE, IN MUNDARI AT LEAST, THE MAIN FACTORS DETERMINING THE GRAMMATICAL FUNCTIONS PERFORMED BY THE WORDS OF A PROPOSITION. (emphasis ours – NE & TO)

Before concluding our response to H&R, we would also like to correct a couple of statements in their commentary that misrepresent our position.

In their Section 4.2.1 they bring in the issue of verbs that can be used directly in complement clauses (their examples (8) and (9), = our examples (37) and (38)). In their discussion of why we do not take these as examples favouring a precategorial analysis they pass from a formulation that we would accept – that "here the lexeme is not used in a prototypical function" – to one that we would not – that "i.e., it is not used to refer to a physical object". We take it as accepted practice in the analysis of word classes that the meanings of words under classification should not be taken into account in the initial stage of establishing word classes, and should only be brought in at the stage of deciding what to name the classes (cf. Evans 2000a: 709–710), and at no stage in our analysis does the denotation of a word play a role in determining what class it belongs to.[7] We do, however (Section 3.1), require that the words we are interested in should show distributional equivalence in canonical constructional contexts, so that it is not enough for words to be distributionally equivalent in

7. Apart, of course, from the issue of requiring regular semantic contributions by constructional context across all members of a putative class, but this concerns relations between denotations in grammatical contexts, not the denotations themselves.

grammatical contexts which we know from crosslinguistic work (e.g. Hopper & Thompson 1984) tend to blur the combinatoric profiles of nouns and verbs. The contexts at issue here – complements of phasal predicates like 'finish' or psych-predicates like 'be tired of' – are of this type, and it is for this reason that we do not regard these contexts as establishing a lack of distinction between nouns and verbs in Mundari.

A final, minor point regarding H&R's suggestion (Section 5.3.2) that the Mundari plural marker *-ko* should be analysed as a clitic 3rd person plural pronoun. While there is undoubtedly an etymological relationship between the nominal plural suffix *-ko* and the 3rd person clitic pronoun *=ko*, we need to distinguish the two analytically: the first attaches at the right edge of NPs, while the second attaches at the right edge of the constituent preceding the predicate or, if no such constituent is present, to the right edge of the predicate itself (see our example (24b)).[8] The distinctness of the two is shown by the fact that they can co-occur one after the other, as in our example (24a). And the absence of examples in which the nominal plural marker – as opposed to the subject pronominal clitic – attaches to a non-noun, casts doubt on H&R's suggestion that Mundari is a "transnumeral" language.[9]

Though we have taken issue with the notion of a "flexible" language as a coherent construct in the typology of word class systems with little or no distinction between nouns and verbs, we want to emphasise that this in no way denies the typological interests of the sorts of phenomenon H&R focus on through this term. H&R are right to point out that is an interesting crosslinguistic parameter how far languages permit, or discourage, zero conversion. How far this interacts with other typological features, what proportion of the lexicon it affects, and what sorts of generalizations we can detect in the interaction of base lexical

8. Incidentally, H&R are not accurate in characterizing *-ko* as restricted to animate entities: inanimate examples are *lijaq-ko* 'cloths' and *parkom-ko* 'beds'.

9. Consider the adjective *salangi* 'tall'. It is impossible to follow this directly with the plural marker *-ko*: **salangi-ko=ko kamitana* is not an acceptable way to say 'the tall ones are working', and one needs to add a head noun like *hoRo* 'man' to make this acceptable: *salangi hoRo-ko=ko kamitana*. Adjectives may in fact be substantivized by zero conversion, but take an abstract meaning: *salang* 'adj. tall; n. tallness' etc.
The only basically adjectival word, *marang*, which can be followed directly by plural marking *-ko* is *marang* 'big', but this is a very special use, with the particular meaning 'older brothers/sisters'. Thus it is possible to say *marang-ko=ko kami-ta-n-a*, adding *-ko* to *marang*, which is basically an adjective meaning 'big'. However, the meaning of this sentence is more specific: 'elder brothers/elder sisters are working', and not simply 'the big ones are working'. Because of this specific semantics we would argue that this is a particular case of a deadjectival nominal, rather than of the addition of *-ko* to an adjective.

meaning with added meaning are all interesting questions worthy of detailed research.

4. Croft

Unlike H&R, Croft accepts our arguments that Mundari does have word classes. His disagreements with us run in other directions, and we will focus below on four main points. We do not address his criticism that we have not come up with a rigorous theory of universals, with regards to parts of speech: this was never our goal, which was the more modest one of setting up agreed rules of argumentation that need to be satisfied before the analyst can claim to have established a lack of differentiation between major word classes.

The four critical points we will address below are the following:

(i) that in our principles (i) and (iii) we are simply applying the distributional approach developed by the American structuralist tradition and its generativist successors, with all of its drawbacks;

(ii) that we do not give any principled account of how to weight particular distributional criteria, or of how to distinguish between higher-level classes (e.g., verb) and subclasses (e.g., transitive verb);

(iii) that we have not shown how the word classes which we have established can be labelled "noun" and "verb" in a way that enables comparisons across languages – which leads into fundamental questions about the crosslinguistic comparability or otherwise of the categories established on intra-linguistic grounds, and about whether it is in fact possible or appropriate to generalize about large-scale classes rather than associating each individual lexeme with its own distinct combinatoric profile;

(iv) that the semantic contributions we permit constructional contexts to make are too permissively characterised.

(i) Relationship of our principles to distributionalist approaches. Certainly, our arguments are neo-classical in the sense that we wish to retain the strengths of the structuralist tradition. Chief among them is the use of distributional evidence to establish word classes in a way that avoids "notional definitions" which second-guess the question of which meanings are associated with which classes, by assigning words to classes on the basis of their meaning rather than their combinatorics, or on the basis of what class they belong to in some reference language (Latin, English etc.). The wish to preserve this advantage lies behind our requirement of distributional equivalence (Section 3.1), including the requirement of bidirectionality (Section 3.3).

However, the form of distributionalism pioneered by the American structuralist tradition had a serious flaw: It considered the study of meaning unscientific and generally avoided the use of semantic criteria. The principle of

compositionality that we formulate in Section 3.2, including the corollary of compositional consistency, makes crucial reference to meaning.[10] This is not a feature of the American structuralist tradition, but grows out of the more confident approach that linguists now take to meaning, regardless of whether they come from a formal semantic background (and the principle of compositionality has its source in the "rule-for-rule" hypothesis of Montague semantics) or from a background in cognitive semantics, with its interest in lexical polysemy.

Bringing semantics into our heuristics in this way has two main advantages: it avoids certain types of undue proliferation in distributional profiles, and it assists in the crosslinguistic matching of word classes set up for each language on language-individual grounds. We consider the first advantage here, returning to the second below.

As Croft points out, Bloomfield (1933: 269) and Harris (1946: 177, 1951: 244) were concerned that, if applied rigorously, the distributional method would end up splitting a language's lexicon into a myriad of tiny classes, asymptotically one class per lexeme.[11] Introducing semantics into our heuristics allows to shave off a major cause of apparent distributional chaos, which results from the differential effects of polysemy on the distribution of words.[12]

Compare the distributions of SEED and PETAL in English. If we cannot refer to meaning, we cannot distinguish the signifiers *seed*$_1$ 'flowering plant's unit of reproduction or germ capable of developing into another such plant' and *seed*$_2$ 'remove *seed*$_1$s from (fruit)'. As a consequence, we get an apparent distributional difference between PETAL and SEED: the former combines with

10. Note in passing that this is not going back on the decision not to employ notional definitions, since we are talking here about the relations between meanings in different constructions, rather than using word-meanings to assign words to classes directly.

11. In his discussion of Bloomfield and Harris in Section 3, Croft also mentions Gross (1979) as giving "an empirical demonstration of this fact" (i.e., of each word belonging to its own word class). This is a misrepresentation of Gross's position. Gross happily employs the categories of noun and verb, but his argument is directed towards establishing that different French verbs pattern distinctly in terms of their profile of combination with a range of complement types. In other words, he accepts the validity of noun and verb as major word class categories (to which one can, for example, apply the usual tests in terms of the combinability of nouns with articles, and plural suffixes, and of verbs with auxiliaries, and inflectionally encoded person/number/tense/aspect categories), but wishes to argue for the subcategorization of verbs into distinct subclasses when it comes to the more subtle distributional facts that one discovers once one looks at patterns of complementation.

12. We deliberately use 'words' loosely here for lexical stems that may prima facie be linked, since this is the point of departure for the sorts of analysis we are considering. Exactly how many lexemes one ends up identifying depends on one's lexicological assumptions about how to deal with heterosemy and polysemy; in our seed/petal example we use small capitals for what might be grouped together by initial hypothesis (treating meaning in the grossest possible way), and italicized words with numerical subscripts for the sort of final groupings established after separating distinct lexemes in a relation of heterosemy.

articles and the plural suffix *-s*, but not with auxiliaries or the continuous suffix *-ing*, while the latter combines with both. But once we admit semantics to our treatment, we split SEED into (at least) two signs, in a relation of heterosemy to one another, and $seed_1$ and *petal* are now seen as distributionally equivalent, since $seed_1$, like *petal*, cannot combine with auxiliaries of the continuous suffix *-ing*. The apparent distributional difference was due to us lumping two signs together into one.

(ii) Weighting of distributional criteria; subclasses. On Croft's view, the application of distributional criteria would lead to a fragmentation into tiny classes – "The result of this is that one does not end up with large word classes, as E&O want. Languages simply do not behave this way" (Section 3) – unless the analyst resorts to "methodological opportunism" by arbitrarily elevating a canonical subset of distributional facts. He criticizes us for falling prey to this malpractice in our analysis of Mundari.

Both sides of this claim strike us as very odd, running against the results of grammatical and lexicographic descriptions carried out for hundreds if not thousands of the world's languages, from English to Russian to Japanese to Kayardild. These descriptions do not simply "grab a construction here, a grammatical inflection there". Although the exact criteria they use differ from language to language, the distributional tests are not a random grab-bag. A rather restricted set of morphological and syntactic criteria recur in language after language (see, e.g., Schachter 1985), even if the exact set instantiated in a given language varies, and we sometimes stumble on exotica.

We also have an ever-clearer idea of which distributional facts predict which others – no-one disputes that incorporation contexts, for example, are a less accurate way of finding out what categories a noun inflects for, than occurrence as the head of a free NP. In other words, some properties of members of a particular class turn out to be excellent predictors of other properties, so that one can set up chains of dependencies between properties, rather than there simply being a rag-bag collection of randomly intersecting characteristics. These chains of dependencies can then be compared meaningfully across languages (cf. Plank 1984).

Moreover, the tests correlate clearly with Croft's typology of "propositional acts", which is why, in so many languages, definiteness turns out to be a good criterion for nouns, tense and aspect turns out to be a good criterion for verbs, and why comparatives turn out to be a good criterion for adjectives.[13] Croft

13. Of course, neo-structuralist procedures are flexible enough to handle "unexpected" correlations – tense on nouns in Kayardild or Guaraní, or object definiteness on verbs in Hungarian. Our point is that the weighting of some criteria over others is not arbitrary, but motivated by their links to basic clause functions.

has, himself, written persuasively of the rationale for many of these effects, and in so doing has made explicit some of the typological reasons why particular criteria work the way they do. But the judicious privileging of some criteria over others is something with a long history in both descriptive linguistics and typology, and dismissing it as mere "methodological opportunism" overlooks the many perfectly defensible reasons – admittedly not always made explicit – for the choices that are made.

The other part of Croft's criticism concerns the rationale for setting up a nested hierarchy of classes and subclasses, which may also allow for multiple combinations of dimensions, e.g., transitivity of verbs on the one hand, and their aspectual properties on another. Here too, we would argue, standard descriptive practice proceeds from a fundamental and defensible assumption. This is the logical ordering of distributional properties from general to particular, which is used to set up a hierarchy such that subclasses inherit distributional generalizations from superclasses, adding in further distributional criteria of their own. In both Russian and Kayardild, for example, "inflectability for case" establishes a superclass (sometimes called nominals) containing nouns, pronouns, demonstratives, and adjectives; nouns then inherit this property but introduce further properties of their own.

Likewise verbs in all four languages have their own profile of inflectional possibilities, but subdivide into transitive vs intransitive (inter alia), dynamic vs. stative in Japanese, and so forth. Again subclasses inherit the distributional characteristics of verbs as a whole, but add additional specifications characteristic of their subclass (e.g., particular limitations on which inflections are possible with adjectival verbs in Japanese). Certainly some words end up with a unique or highly distinctive distributional profile – *wuuja* 'give' is the only Kayardild verb allowing five distinct case frames (Evans 1995: 334), just as *rent* is the one of very few English verbs that is self-converse, allowing for the rearrangement of prepositions: *Bill rented his bike from Kees* ↔ *Kees rented his bike to Bill*. But this does not stop them behaving, at the level of higher-level generalizations such as their combination with inflectional suffixes or (in English) auxiliaries, exactly like the thousands of other members of their major word class.

For these reasons, we see the prospect Croft holds up – of a mapping of distributional properties that "does not produce a large-scale, exclusive partitioning of the items that fill the roles" and for which there is instead "a complex, many-to-many mapping between constructions and elements" (Section 3) – as a programmatic chimera. While we would not rule out the possibility that a language whose grammar looks like this exists, we are not familiar with a well-worked out description of any language – in the form of a grammar plus lexicon – which employs this sort of treatment, or which shows why it is necessary.

(iii) Equivalence of noun, verb, etc. across languages. To begin with, we point out that it was not the purpose of our paper to set out criteria for how major word classes should be matched up across languages. Rather, it was to make explicit the argumentative steps that need to be gone through before establishing that no distinction between major word classes exists. Failure to satisfy these steps merely results in the positing of two (or more) major word classes; what they should then be called is a separate and complex issue.

Nonetheless, we believe that once we readmit semantics to our heuristics – and again this is in no way an original claim, having been made by many scholars before us – we get a lever for setting up cross-linguistic equivalences of (some) word classes, something that could not be done under classic structuralist methods. In our view – and we are in fact largely drawing on the same assumptions as Croft here – there are three types of procedure for doing this: PROTOTYPICAL DENOTATIONS, PROPOSITIONAL ACTS, and CONVERSION EFFECTS. Between them, they allow us to address Croft's questioning of the validity of us naming the classes we have set up in Mundari as "nouns" and "verbs".

The first procedure, pioneered by Dixon (1977) in his study of adjectives, developed by Croft himself in a number of publications (e.g., Croft 1991), and implicitly accepted in our article here, is to line up the distributionally-established classes from each language on the basis of what their prototypical denotations are. Here the assumption is that while the boundaries of the category may vary ('intelligent' may or may not be encoded as an adjective), the meaning of core members is crosslinguistically stable. If a language has an adjective class, it will include 'big' and 'good' as meanings, and conversely an established class with 'big' and 'good' as core members can safely be labelled "adjective" whether or not it includes a word meaning 'intelligent'. Though Croft, in his discussion of our article, links these "prototypically correlated lexical semantic classes" to "propositional acts" rather than to distributional classes, in fact we can typically argue for a three-way linkage between word classes, propositional acts, and ontological types (e.g., nouns <> reference <> objects) as argued in Croft (1991). It is on the basis of these considerations that we give the labels "noun" and "verb" to the categories we set up in our analysis of Mundari.

The second procedure, which has become such common practice among typologists (including both Hengeveld and Croft) that it is difficult to associate it with any particular investigator, is the use of what Croft labels "propositional act" functions: REFERENCE, PREDICATION, and MODIFICATION. Marrying this with analyses that permit us to identify marked and unmarked uses of lexemes then gives us a principled basis for regarding some distributional facts as more fundamental than others, a point we return to below.

The third procedure, developed by Croft, builds not on the basic denotations of words, but on the semantic and grammatical effects that constructional coercion produces on them, as outlined in his (1).

In each case, combining semantics with distributional arguments gives us a firm basis for identifying word classes across languages. It is for this reason that we see it as helpful to produce a set of criteria that draws on the classic distributional methods of the structuralists, but augments them with formulations that draw on semantics where appropriate.

(iv) Directionality of semantic shifts. Although Croft agrees with our criticism of approaches that allow idiosyncratic semantic shifts, he takes us to task for not tightening up our criterion (ii) to rule out particular directionalities to any regular shifts that take place as lexemes occur in particular constructional contexts. In particular, he claims that while our criterion allows for languages "in which a predicated action word means 'do Verb' and the same word in a referring expression means 'one that Verbs"' – a pattern attested in a number of languages – it also allows for languages "in which a predicated action word means 'be one that Verbs' and the same word in a referring expression means 'the act of Verbing"' – an unattested pattern. He goes on to give plausible typological reasons why this pattern should never be found.

Is the failure to tighten up our formulation so as to rule this out a mistake? We do not believe it is. The goal of our paper was not to test universal claims, but to formulate rules FOR THE DESCRIPTION OF INDIVIDUAL LANGUAGES, which can then be fed into the crosslinguistic data base over which universals are tested. If the principles used in constructing individual descriptions would prevent us from recognizing such a language, then the lack of its attestation has no typological significance: it merely follows (quasi analytically) from our descriptive procedure. On the other hand, if the rules of argumentation do not themselves exclude that one could produce such a description, but nonetheless no such grammars are found, then we have a significant typological fact in need of explanation. For this reason we would not wish to revise our formulation, and in fact see it as an advantage that it permits the sort of grammar whose lack of attestation Croft notes.

To complete this part of our response to Croft we would like to underline our endorsement of the final challenge he lays down to investigate the word classes of Mundari in more detail than we could here. A rigorous statement of constructional meaning for the various constructions we have discussed, and a categorization of an appropriately large number of lexemes for their behaviour and meaning in each of these constructions, and more – rather than just a judgment of acceptability and meaning as "noun" or "verb" as we have done here – would be a huge advance on the analysis we have presented, though it is no small task.

5. Conclusion

Linguistics still has a long way to go before we can claim to have a generally applicable set of analytic practices that on the one hand are rigorous enough that evaluating them is not just a matter of personal taste, or of adherence to a particular theoretical school or areal tradition, and on the other are supple enough to capture the full range of diversity found in the world's languages. Typologists need convergent rules of argumentation at least as much as any other sort of linguist, since otherwise they will never be sure whether the differences in the descriptions of languages they are comparing reflect true differences between the languages themselves, or mere differences in analytical assumptions on the part of their grammarians.

Recent years have seen considerable convergence in descriptive and analytic practices, including steps towards a standardized glossing system (since Lehmann 1982) and a unified ontology that must underly it. We have made less progress in standardizing the practices of argumentation,[14] yet until we make these explicit we will be left with a situation where what counts as evidence for one linguist will be deemed irrelevant by another. This leaves our field roughly where microbiology was before Koch's postulates laid down guidelines for how a researcher demonstrates that infection by a microbe causes disease.

Because the assumptions that underly argumentation are so numerous, and interact in so many ways, developing a set of convergent rules of argumentation is a huge task for the field. The very different responses of our distinguished commentators show how far we still are from having an agreed set of rules of argumentation within word class typology. Croft accepts our arguments that Mundari does not have just a single major word class, while Hengeveld and Rijkhoff reject them; Croft takes us to task for being too permissive in the semantic shifts we allow lexemes to accrue within constructions (e.g., interactions with aspect) while Hengeveld and Rijkhoff reject the constraints that we require. As so often in linguistics, whether an analysis is considered convincing depends on the assumptions one brings to its evaluation, and we will not have generally acceptable principles of argumentation until we have agreement on what those assumptions are. We hope that the principles we have outlined, both

14. There have, of course, been periods in linguistics where the need for these has been discussed particularly overtly – the American Structuralist period, for example, and the early phase of generative grammar (as exemplified by the focus on argumentation in Soames & Perlmutter's (1979) textbook). A renewed interest in generalized rules of argumentation has much to draw on, thanks to this prior work. However, it must now take into account a number of other crucial developments in the field, in particular the integration of semantics, the distribution of domains of interest across different schools (e.g., polysemy and figures of speech to so-called cognitive linguistics), and our vastly increased knowledge of the world's languages.

in the original article and in this response to the thoughtful critiques proposed by our commentators, will help to clarify this process.

Received: 19 September 2005
Revised: 6 October 2005

University of Melbourne
Research Institute for Humanity and Nature, Kyoto

Correspondence addresses: (Evans) Linguistics & Applied Linguistics, University of Melbourne, Parkville, Victoria 3010, Australia; e-mail: nrde@unimelb.edu.au; (Osada) Research Institute for Humanity and Nature, 335 Takashima-cho, Marutamachi-dori Kawaramachi nishi-iru, Kamigyo-ku, Kyoto 602-0878, Japan; e-mail: osada@chikyu.ac.jp

References

We only give those references not already cited in the main article or the commentaries thereon.

Dryer, Matthew (forthcoming). Descriptive theories, explanatory theories and basic linguistic theory. In Felix Ameka, Alan Dench, & Nicholas Evans (eds.), *Catching Language: The Standing Challenge of Grammar-writing*. Berlin: Mouton de Gruyter.

Evans, Nicholas (1995). *A Grammar of Kayardild*. Berlin: Mouton de Gruyter.

Evans, Nicholas & David Wilkins (2000). In the mind's ear: the semantic extensions of perception verbs in Australian languages. *Language* 76: 546–592.

— (2003). Culture and structuration in the languages of Australia. *Annual Review of Anthropology* 32: 13–40.

Hopper, Paul & Elizabeth Traugott (1993). *Grammaticalization*. Cambridge: Cambridge University Press.

Lehmann, Christian (1982). Directions for interlinear morphemic translations. *Folia Linguistica* 16: 199–224.

Plank, Frans (1984). 24 grundsätzliche Bemerkungen zur Wortarten-Frage. *Leuvense Bijdragen* 73: 489–520.

Soames, Scott & David Perlmutter (1979). *Syntactic Argumentation and the Structure of English*. Berkeley: University of California Press.